Conference

on

Guidance Theory and Trajectory Analysis

November 17, 1965

Electronics Research Center

National Aeronautics and Space Administration

PREFACE

The papers in the following documents were presented to representatives of industrial and educational institutions on November 17, 1965 by members of the ERC Guidance Laboratory.

The primary purpose of the conference and this document is to develop the close communication between the Center personnel and the scientific community which is necessary to successfully fulfill the mission of ERC. The technical content of the papers should be viewed as reflecting the current plans of our staff. Obviously, these will change according to the needs of the overall NASA program. We welcome any contributions which will assist us in extending knowledge in this important area.

Winston E. Kock

WINSTON E. KOCK
Director

GUIDANCE THEORY AND TRAJECTORY ANALYSIS CONFERENCE

DATE:	November 17, 1965
LOCATION:	MIT - Humanities Library Lounge Building 14E Room 310
SESSION CHAIRMAN:	R. J. Hayes Acting Chief, Guidance Laboratory

AGENDA:

1:00p.m.	Opening address by Dr. R. Langford, Asst. Director for Guidance, Control, and Systems
1:15p.m.	Research Goals and an Integrated Approach - R. F. Hoelker, ERC
1:45p.m.	Research in Celestial Mechanics M. Payne, ERC
2:30p.m.	Intermission
2:45p.m.	Research in Astrodynamics and Computer Exploitation - N. Braud, ERC
3:30p.m.	Research in Optimization & Guidance Theory - D. Schmieder, ERC

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GUIDANCE THEORY AND TRAJECTORY ANALYSIS CONFERENCE

November 17, 1965

RESEARCH GOALS AND AN INTEGRATED APPROACH

by

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You will hear four talks now. The first one will define the technical areas we are concerned with in the Trajectory Analysis and Guidance Theory Group, and will bring out the development trends that seem best to respond to the requirements of the future decades of space flight.

This talk will break down the technical areas into scientific disciplines and sketch out the ways how these disciplines contribute to the advancement of the technical areas.

This speaker will also elaborate on the mechanism by which we hope to carry our research program through in close cooperation with the scientific community.

The talks, following mine, will take a close look at four major disciplines and formulate problems of major importance.

When I now start to develop research goals which we plan to aim at in the coming years, we have to have these research goals correspond to demands that will exist when our research results will be implemented. So let us look at the years ahead, maybe 20 years from now, to see what shape our space activities will have assumed by that time. Let us particularly focus on the flight operational aspects, because it is from this side that new requirements arise for the guidance research program.

We will have missions to the far planets, frequent flights, maybe a regular traffic pattern, to the moon and near planets. We may have galactic probes on the way. Permanent space stations in the earth-moon space and perhaps in planetary space will be cruising as navigation satellites, as communication links, maybe as wayside stations for emergency depots, and as space laboratories for scientific research of various types.

The density of flights, especially of manned flights, on one end of the spectrum and the extremely long travel times on the other end, will have called for operational techniques that strongly deviate from those of today, which are essentially single mission oriented. To condense the time that a program will take for exploration of distant bodies, flights will have to be telescoped by launching follow-on probes, before the first of a chain will have reached its destination. (This pattern tends also to mitigate the communication problem over long distances.) The follow-on probes will be given new assignments or diverted to other missions if the earlier probes have been successful. The reliability of the mission will be increased greatly through the development of advanced methods of onboard failure detection and automatic correction. In manned missions, in-flight changes will occur, be it for the safety of the crew or in support of other manned space vehicles, but also to enhance the payoff of the mission itself. The authority of deciding changes of in-flight plans may have to rest with the astronaut.

This degree of flexibility and independence of flight operations can be achieved only if adequate developments will have preceded in the navigational and guidance field.

This concerns particularly methods for fast and efficient onboard computation of optimized flight-profiles and the real time generation of the steering commands. The astronaut should be able to interrogate the computer for any detour or new flight plan and receive not only the optimized course, but also an evaluation of this with respect to propellant expenditure, staging requirements, sensitivity behavior, and possibly also probability of success.

While this calls for the development of highly sophisticated methods and a reliance on the computer's capacity, there is on the other hand the need for increased reliability of the flight. This requires the simultaneous development of alternate and simplified methods that either function still with partial failure of the computer or, on the lowest level, do not rely at all on the spacecraft's computer. In the last case, either information by communication with other spacecraft, or with ground, can be considered, or more likely to be useful, the development of methods that are simple enough to be performed by the astronaut himself aided by nothing more than a slide rule and perhaps some tables. While it is obvious that the operational use of such a simplified method is the last recourse in an emergency situation, the potential use of it for checking the computer's output and thereby increasing the astronaut's confidence should not be overlooked.

We notice, therefore, that requirements arise on one hand for highly powerful guidance solutions that serve to make flights flexible and operationally independent, and on the other hand, for various levels of guidance solutions whose simultaneous availability will greatly increase the reliability of the flight mission.

Our research program is to be aimed at meeting these operational requirements.

Let me define now specifically the technical areas, we are concerned with here, and list those disciplines that will contribute strongest to the development of these technical areas.

When I give you definitions, I am well aware that there are almost as many different definitions of guidance terms as there are experts in the field. This very fact, however, demands that the meaning of the terms used here be defined. I trust that you accept the definitions for the length of the conference to have a common platform to base our discussion on.

A distinction between navigation and guidance will be observed, which assigns navigation the role of deriving and determining the current state variables, in flight or on ground, and associates the term "guidance" with the operations of determining the path-ahead and of giving instructions to follow this. This distinction recognizes a continuous surveillance of the flight-path in comparison with the mission requirements and assumes the availability of onboard means to change course at any time in flight. It does not exclude that at times of free flight the navigational data, if furnished in appropriate variables, describe the path-ahead as well.

A distinction is made between guidance and control which is manifested in cutting the role of guidance at the point of "giving instructions." This excludes from guidance the operations that execute the guidance commands. The loop that executes e.g. the vehicle attitude commands and deals with the rigid body dynamics of vehicle and control organs is associated with "control" or "attitude control."

After the term "guidance" has been isolated from navigation and control, we can approach the term "guidance theory" as used here today. I proceed from the part to the whole. Steering commands and thrust switching commands are generated as functions of state variables or time or other variables. To express the functional dependence, we use the terms "steering functions" and "switching functions" or collectively "guidance functions." This term can now as well be used to describe the mathematical equations that express the functional relations. When doing this, the theoretician looks for two things. First, the method by which these equations are derived, be it for instance by deduction from physical laws or by empirical means. Secondly, he asks for the principle or merit factor, governing the derivation, be it an economy viewpoint as applied in optimization methods or the more modest feasibility viewpoint as referred to in empirical approaches. The application of these two criteria to the guidance functions and the corresponding classification of function generation leads to the concept of "guidance policy" or "guidance mode." Finally, we arrive at the description of guidance theory as the study of guidance modes.

You will agree with me that guidance modes can be studied at great length without considering any hardware systems for their implementations. In fact, a good guidance mode fits all reasonable systems, be they self-contained or ground linked. This separability explains the use of the term "guidance theory" in the defined sense, though it goes without saying that theoretical aspects are permeating any hardware design and analysis.

In a parallel form of abstraction, a navigation theory may be defined as the study of mathematical methods applicable in deriving information about the current state from a set of given conditions. The diversity of conditions and the associated value functions do not allow the degree of independence from hardware systems for this theory as it is possible for guidance theory.

We know well enough that both concepts are results of idealizations and that in the implementation stage, the real world will ask for a lot of compromises.

Returning now to the requirements of future space flight, as expressed in greatly increased flexibility and reliability, we believe that essential contributions on the guidance side are coming from developing more efficient and more powerful guidance modes.

How will we go about? Here, let me discourage one line of thought. We will not favor the empirical approach, which uses arbitrary simplifications and fittings for which the range of validity cannot be analyzed. These approaches, however well they may give solutions for a specific mission, are deadend streets for the overall mission needs. I would like to state even more specifically that we are not primarily looking now for the development of a single guidance mode, but rather for development of methods that are inherently fruitful for later applications to a guidance mode.

What does the astronaut really ask for if he interrogates the onboard or ground computer for generating the guidance commands. In order to generate the appropriate guidance function, the computer must implicitly solve for the optimum flight profile between "now" and the destination. In cases where the astronaut wants a visualization of his flight profile and calls for a display, this computation is to be quite explicit. The expected large variety of flight changes and excursions then, in fact, demands that onboard, in almost real time, all that be done what today is done on ground in preparation of a flight in tedious hours of computational work.

Thus, guidance research is not accomplished in hunting for some ingenious computational manipulations, that under lucky circumstances bring the spacecraft to its flight destination. Rather, guidance theory aims at guidance modes that represent in extremely compact form, the integrated results of many trajectory studies.

To develop such advanced guidance modes, we have, first of all, to look at those disciplines that are the main constituents of trajectory analysis and trajectory design. Since flight profiles involve free flight and powered flight arcs and these are to be composed in some optimum manner, we have to look at: celestial mechanics, astrodynamics and optimization theory. The fourth discipline is that of numerical analysis in application to high speed automatic computers.

Allow me to give you again some definitions, just to avoid misinterpretations. Let us understand then that both, celestial mechanics and astrodynamics, are concerned with deriving information about free flight arcs. Of these, we assume that celestial mechanics aims at obtaining insight by means of analytical processes, and that astrodynamics resorts primarily to computational or numerical means for deriving information.

I will not enter a detailed discussion of research programs now, nor will I do this for the other disciplines listed; the speakers following me, I trust, will do this in an enlightening fashion.

However, what I would like to do is to list briefly a few sample problems from each of the disciplines. The list is not intended to be exhaustive nor to be classifying. It merely should serve to give you, at this point of our discussion, a general idea of what we are thinking of.

First, in the field of celestial mechanics, quite an important part of our future studies will be devoted to the development of new mathematical techniques for representation of free flight trajectories between two or more celestial bodies. A close look will be taken at the evaluation of older techniques of classical celestial mechanics with modern means of computation. The behavior of single trajectories as well as the large scale behavior of classes of solutions are of interest. The search for additional integrals of motion is among the outstanding problems.

In the second field, that of astrodynamics, extensive numerical studies are well underway at many institutions. Here, the element of organization is strongly missing since hardly anybody aims at building up knowledge in the large, which could result in global solutions of two-point boundary value problems. Here we are confronted with the problem of data-management and a sensible representation of the results, which are of multivariant functional character.

Thirdly, in the area of powered flights, classical methods of optimization as well as the more recent developments in this area have to be furthered to deal with such problems as non-linear constraints on state and control-variables. The simultaneous solution of a split boundary value problem in the sense of the Denbow Theory is to be developed especially in applications to interplanetary profiles. Also in this area, the attempts to gain qualitative information will be pursued to obtain insight into the structure of classes of solutions, their stability behavior, and possibly to acquire information of global character.

The area of low thrust trajectories deserves an increasing level of effort. Method of perturbation theory as developed in classical celestial mechanics will be explored particularly here.

Fourthly, in the area of numerical analysis, our particular attention is to be directed to the advancement of techniques that are contingent on the existence of large automatic computers. Automatic processing of formulas in the realm of algebra or calculus is being developed; but it experiences difficulties of various nature. Especially the problem of reduction is unsolved today. Efficient automatic pattern recognition techniques are needed for this process. These techniques will also play an increasingly important role in at least two other areas. Stepwise integration procedures, e.g., could be made much more efficient by adapting continuously to the functional character of the solution. The other area is that of constructing functional models to fit an array of numerically derived data. This is encountered in all cases where large scale information is desired and a representation is sought of the mass of data in a way that is "best" in some sense. The before mentioned problem of deriving information about the global behavior of a function will greatly benefit from the automatic generation of functional models.

You recognize that solutions to any of these problems will constitute progress for guidance theory.

I might conjecture that at the far end of the development line, we will have methods of trajectory calculations that are so compact and powerful that any flight problem can be solved by an onboard computer. This is the time when we cannot justifiably speak of a distinction between trajectory calculation and guidance theory. Before this is achieved, the problems that are typical of guidance theory in contrast to the common problems, are concerned with onboard computations, that is, those of time and space constraints. These will be dominant in the near future and compromising solutions will be sought. Interim solutions, however, should always be developed as steps and milestones of the long range development program. This means that, on one hand, all current progress should be integrated in a new mode and on the other hand, the guidance mode should inherently have the growth potential in the direction of our end goal.

Before we conclude this part of the discussion, I have to mention one area that accounts for another sector of our activities; that is the area of mission support. Since in the Center, our group is the only segment that specializes in trajectory analysis, this group is expected to lend a great deal of support to other segments of the Center in studies that require input from this field, as in system design, error analyses, navigational studies, and others. In line with these duties, support will be required from outside sources, industrial as well as institutional, for the preparation and coding of computer decks and for trajectory and simulation studies.

Mentioning this leads me now naturally to the last part of my address, that is the discussion of the managerial aspects of the program.

To achieve the goals of this ambitious program solely by in-house research is neither possible nor in the spirit of the charter that the Center received with its establishment.

It is the Center's desire to call on the scientific community for a strong support of this program.

This support should extend over all phases of the program. So in the conceptual and layout phase, we like to probe your thinking and have your advise concerning the most promising approaches. This is an open invitation to speak out, if you feel strongly in one or the other direction.

When the program is underway, there will be frequent cases when we like to call on the experts to evaluate with us the results obtained or appraise with us the progress currently made. Though there may be particular procedures by which we organize and mechanize these consulting activities, a well suited vessel of communicating your opinion is the institution of the regular seminars which I shall mention shortly.

Next then, we have to talk about the mechanism of the scientific and technical contributions to the research programs.

Considerable support by outside institutions will be required. Here we address with equal intensity industry and educational institutions. The character of the program consisting of research of advanced nature makes it almost mandatory for us to look for the specific man rather than the organization. Some contracts are by the nature of the research oriented about the man with the brilliant idea.

Our process of soliciting is the conventional one. Letters of interest are published in the pertinent news media, and succeeding this, requests for proposals are sent out. This is the procedure applied to industry. One means to reach the non-profit organizations is the recently instituted publication of "Research Topics Bulletin" which is managed by the Office of Space Sciences & Applications of NASA Headquarters.

The nature of the program, consisting of contributions from various sources that are sometimes lying far apart, with respect to disciplines, makes it a necessity to coordinate the efforts carefully. This involves on our side steps as the following: pointing out to the isolated scientist where his contribution fits into the overall program, showing interfaces between seemingly unrelated areas, stimulating responses to results of other researchers, creating groups of before unrelated research areas for mutual stimulation, common orientation and working direction, closing existing gaps, and fostering new ideas and concepts.

To fulfil this list of duties, quarterly meetings are planned, where all contributers will be present. There will be two levels of discussions. A meeting of all scientific contributers will be held where those research studies that have progressed to a level of preliminary or final results will be presented to the group for information and general discussion. Here new concepts of general interest may be brought up for consideration. We will also have invited addresses by experts. Secondly, smaller group meeting will be held that are mainly concerned with single disciplines. Here, new ideas find their first testing ground, particular difficulties encountered are discussed, and new directions are agreed upon.

During these meetings, there will be sufficient space and time for the researchers to meet individually for exchange of ideas.

We emphasize that a certain character of informality will be maintained. That is why we prefer to speak of seminars and not of conferences. We plan to hold the seminars as open sessions.

Bi-annual publications are planned to consolidate the research accomplishments and related seminar talks.

Concluding my talk, I would like to express the sincere hope that the Center succeeds in attracting the very best in the scientific community, and that the common attack on the problems of spaceflight will lead to the tangible results we need for the coming decades.

GUIDANCE THEORY AND TRAJECTORY ANALYSIS CONFERENCE

November 17, 1965

RESEARCH PROGRAM FOR CELESTIAL MECHANICS

by

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In this presentation, I should like to review with you some areas in which research in celestial mechanics is indicated and the initial directions in which our research effort will be oriented. Before outlining our program, I should like to make a few background remarks. We might say that the primary aim of our research in celestial mechanics is to obtain the solutions, in various senses, to the differential equations of motion of unpowered space vehicles or, to be more specific, we wish to provide the means to treat initial value problems, boundary value problems and stability problems which arise in the analysis of the motion of space vehicles. Many techniques have been developed to study these and similar problems over the past three centuries. Most of these methods are limited in their application. Many have difficulties involving convergence, small divisors, and precision estimates. Most were developed for the analysis of the motion of the celestial bodies and the most sophisticated deal primarily with near circular, relatively long period orbits which are stable over the time intervals during which they have been observed. With space vehicles, high eccentricities may occur, as well as very close approaches to two or more centers of attraction. In the application of classical theory to these problems, both the convergence and small divisor difficulties become increasingly difficult to live with.

Our approach to the resolution of these difficulties will, we hope, be a broad one. We should certainly first examine the existing methods and determine those problems for which they are adequate. We should also explore the possibility of extension and generalization of the existing methods for application to a larger class of problems. For example, the feasibility for further development of the classical methods treating high eccentricity orbits should be explored. Third, we should exploit existing techniques from other disciplines for application to the motion of space vehicles. For example, the recently developed asymptotic matched conic techniques are based on a method borrowed from the boundary layer theory of aerodynamics. As another example, the Schroedinger perturbation method of quantum mechanics can be used to determine the coordinate transformation (if one exists) leading to separability of the Hamilton-Jacobi equation. Fourth, we should take full advantage of the modern qualitative techniques for the analysis of dynamical systems not only from the point of view of extracting interim information pending the development of explicit solutions, but also recognizing that such methods, in some cases may yield all the meaningful information obtainable from an explicit solution, and in other cases yield, as by-products, techniques for construction of explicit solutions. For example, Sundman's existence proof for a convergent power series solutions to the three-body problem includes a recipe for the construction of such series, albeit a very laborious recipe. Finally, we shall look diligently for entirely new methods of attack. In short, we shall pursue any and all approaches that come to our attention and show promise of contribution to the analysis of the motion of space vehicles.

I think it is hardly necessary to dwell on a justification for research in celestial mechanics in a guidance theory section. One or two comments should, however, be made. As Dr. Hoelker pointed out in his presentation, the ultimate aim of research in guidance theory is the determination of guidance functions for various missions or classes of missions. In a sense, the starting point for the determination of guidance functions is a thorough understanding of the motion of space vehicles under the gravitational influence of the various celestial bodies. Such an understanding is what enables one to make an intelligent selection of approximate models adequate for the analysis of particular problems as well as to select the best mathematical approach for the purpose at hand. It also enables one to use the "natural" forces to the best advantage. For example, it is known that swing-by trajectories can be used to gain substantial fuel savings in interplanetary missions, although sophisticated theoretical analyses of interplanetary trajectories are yet to be developed. In this last connection, one might remark that it is really astonishing that no such sophisticated theories are yet developed for missions that are already in relatively advanced stages of planning. Since the present theoretical techniques of celestial mechanics are in many ways inadequate for the treatment of problems of space flight, there is, thus, a need for fundamental research in this area.

Our approach to fulfilling this need will be primarily theoretical. As noted earlier, our principal aim will be to provide the means for study in depth of the motion of unpowered space vehicles. Our efforts will thus be directed towards the analysis and developments of advanced mathematical methods applicable to this problem.

The field of celestial mechanics is a very old one which, classically, was developed for and restricted to the motion of celestial bodies under their mutual gravitational attractions. In recent years, a great deal of research in this area has been directed to the application of the classical methods to the motion of a space vehicle in the field of one or more celestial bodies and subject also to other natural forces such as atmospheric perturbations or solar radiation pressure. As noted earlier, we shall not be directly concerned with powered flight, although since optimization problems can be given a Hamiltonian formulation, the results of research in celestial mechanics may well, on occasion, be applicable to the theory of powered flight. In fact, since one might characterize celestial mechanics as the study of a special class of systems of differential equations, it may be expected that results of this research will be useful in the analysis of other problems described by similar systems of equations.

The mathematical methods of celestial mechanics are classified into three categories: special perturbation theories, general perturbation theories, and the so-called modern or qualitative theories. Actually, there is some overlap in this characterization and, in many cases, each type utilizes results or techniques obtained from the others. The three categories are as follows:

(1) Special perturbation theories ultimately rely on numerical integration. The crudest such theory is a direct numerical integration of the equations of motion. Refinements of this method may be based on results of general perturbation theory.

(2) General perturbation theories are obtained by a sequence of closed form approximations to the solutions of the differential equations describing the system. In such theories, the two-body problem usually constitutes the zero-order approximation.

(3) The qualitative method does not seek explicit solutions of the problems of celestial mechanics although such may appear as a by-product of the analysis. Its aim, rather, is to obtain information on properties of the solution such as boundedness of the motion, existence and stability of equilibrium points and periodic orbits and, more recently, rates of growth of bounds on the motion.

Our research program has been largely constructed around this classification with, however, due regard to our desire not to limit ourselves to existing approaches. The program is far from complete and this will probably always be so, since we regard the research program as a dynamic rather than a static concept. This presentation would, however, not be complete without some indication of the specific problems which we consider worthy of more or less immediate study. Within this context, I would thus like to list for you some problems of current interest to us with some comments on their significance.

I. SPECIAL PERTURBATION THEORY

The methods of special perturbation theory have undergone extensive development in recent years with the advent of high speed computers. They are probably the single most powerful tool presently available for most problems in trajectory analysis. There are still, however, areas for fundamental studies in this area such as:

1. Generalized special purposes Encke Schemes incorporating more sophisticated models than the two-body problem. Such models might be first on higher order general perturbation solutions.

2. Along similar lines, but eliminating numerical integration, one might attempt long time arc solutions constructed from iterated use of closed form approximations obtained from general perturbation theory.

3. Criteria for "best" regions of applicability of various numerical techniques.

4. Criteria for the "best" selection of parameters for various numerical methods.

II. GENERAL PERTURBATION THEORY

A vast literature on general perturbation theories is available and should be fully utilized. Thus we should certainly undertake problems such as:

1. Development of comprehensive self-contained theories, including all pertinent perturbations, for "orbiter" missions about the sun, earth, moon and most of the planets. The need for such theories is in the immediate future and cannot wait on our long range plans for development of more sophisticated and powerful approaches.

2. Complete feasibility study of the application of existing theory to interplanetary missions involving close approach to two or more gravitational centers of attraction. This too is a problem of pressing current interest as well as a problem basic to the advanced space missions of the future.

3. Further application of existing theory to libration point studies.

The next five items all touch on the concept of integrals of the motion, the selection of the "best" base problem for a perturbation theory and the "best" parameters to describe the base problem. In connection with this last item, we may note that Brouwer's theory of near earth satellites predicts a critical angle of inclination at about 63°. The two-body parameters used in this theory are the Delaunay elements, which are the action and angle variables associated with separation of the Kepler Hamilton-Jacobi equation in spherical co-ordinates. I should like to suggest that with other elements one might avoid this difficulty. For example, consider the Stark effect problem of quantum mechanics, for which the classical analogue is the Kepler problem perturbed by a constant force. Brouwer's theory, using the Delaunay elements, applied to the Stark effect problem yields a critical angle of inclination given by $\cos^2 I = 1-e^2$. If, however, one uses the action and angle variables associated with separation in paraboloidal coordinates, no critical angle occurs, and in fact, the first order solution is the exact solution. This is not surprising if we recall that the Stark effect problem separates and is solvable in closed form in these coordinates. The essential point, however, is that the selection of the Kepler elements is crucial to the efficiency of a perturbation theory based on the Kepler problem. Thus

4. Methods of constructing "new" useful sets of Kepler parameters should be sought.

5. Develop methods of obtaining general solutions to the Hamilton-Jacobi equation. This item has a double purpose. The constants associated with a complete solution of the Hamilton-Jacobi equation are integrals of the motion. The Hamilton-Jacobi equations associated with most problems are not separable. Techniques for the construction of complete solutions of non-separable Hamilton-Jacobi equations would thus be most significant for the direct treatment of such problems. On the other hand, large classes of solutions of the Hamilton-Jacobi equation for the Kepler problem, or other solvable problems useful as base solutions, would make available numerous sets of parameters in terms of which to develop perturbation theories. In much the same spirit is item 6.

6. Development of canonical transformations leading to separability of Hamilton-Jacobi equations.

7. Development of methods for more or less direct construction of integrals of the motion, for example by Poisson bracket techniques. The utility of integrals of the motion can hardly be exaggerated. By an integral of the motion, we mean a function of the dynamical variables describing the problem which is constant throughout the motion determined by a single set of initial conditions. It is evident that many uses can be made of integrals of the motion, not only in obtaining qualitative information and insight but also in numerical calculations either directly or as a check. They are particularly useful in the solution of boundary value problems. It is further known that

for a dynamical problem characterized by n coordinates, a knowledge of n integrals of the motion satisfying certain conditions is sufficient for construction of an explicit solution. The Poisson bracket techniques have been used by Whittaker in the construction of his Adelphic integrals, which usually possess unsatisfactory convergence and analyticity problems. Very recently, Contopoulos has modified Whittaker's method to obtain the first two terms in a series expansion of a new integral for the restricted problem, the only previously known integral being the Jacobi integral. It is too early to evaluate the usefulness of Contopoulos' integral. It is, however, instructive to cite the Stark effect problem again, and note some features of _its_ treatment by this method. Two integrals for the Stark effect problem are obvious: the energy and the component of angular momentum about the direction of the constant force. A third integral is obtainable by separation of the Hamilton-Jacobi equation in paraboloidal coordinates. If one seeks the third integral by Contopoulos' method, the first term in its series expansion must be an integral of the Kepler problem. The "right" selection of the first term leads to a closed form expression in one iteration; the "wrong" selection (which is, incidentally, the obvious one) leads to at least two iterations and most likely to an infinite series. Thus, I should like, once more, to emphasize the importance of the development of methods leading to the"right" choice of parameters in any type of perturbation theory based on series expansions.

8. Study the small divisor problem with particular attention to methods of a priori prediction of the occurrence of small divisors and means of

their elimination. This and the four preceding problems are, of course, different aspects of the general problem of determination of integrals of the motion either directly or by a perturbation theory. Although in-house efforts on several of these problems are underway, we are interested in broadening the scope elsewhere.

9. Construction of analytic representations of the group of canonical transformations. What we have in mind here is something analogous to the three by three orthogonal matrix representation of the group of rotations in three space. This last item under general perturbation theory is along somewhat different lines, although it, too, is linked with the problem of "right" parameters since the group of canonical transformations provides relationships among all possible sets of parameters.

III. QUALITATIVE METHODS

1. Studies on the existence and determination and stability of periodic orbits are currently of interest to a number of investigators. This effort should be continued, enlarged and extended.

2. The same comments apply to libration and near-libration point studies.

3. Criteria should be developed for characterization of interplanetary trajectories. The zero velocity curves of the restricted problem give only limited information, and some more sophisticated concepts would be most useful.

4. Studies on the stability and/or growth of deviations under perturbations in the initial conditions and in the equations of motion. It is expected that an out-of-house effort on such problems will commence shortly.

5. Liapunov's second method has been extensively applied to stability studies in control theory and, to a lesser degree, to stability problems in celestial mechanics. We should like to see it further exploited for such studies in celestial mechanics.

A great deal of research has been devoted to the restricted three-body problem in the past fifty years. Most of the results obtained refer to the planar problem. We should like to see extensions to the work in several directions as indicated in the next three items.

6. Generalization of existing results on the planar restricted problem to three dimensions.

7. Generalization of restricted three-body results to restricted n-body problems (circular motion of the primaries). Even though such problems are not, strictly speaking, realistic models, they may be instructive in providing useful mathematical techniques.

8. Development of generalized restricted n-body theories in which the motion of the (n-1) primaries are described by realistic models.

9. Exploitation of the Sundman power series solution for the restricted problem utilizing symbolic manipulation programs to implement the laborious calculation of the coefficients. This problem will shortly be supported as an out-of house effort.

10. Develop methods for obtaining regularizing transformations, perhaps following Arenstorf's general method for the construction of such transformations for the planar restricted problem. Such transformations could, for example, well play a role in the search for integrals of the motion.

11. Study of the effect of numerical uncertainty in physical constants and in initial conditions on the conclusions obtainable on the character of the motion of dynamical systems. This problem has a philosophical flavor and probably has information theoretic implications. It is important because of the role played by the rationality or irrationality of certain ratios appearing in many perturbation theories.

IV. METHODS FROM OTHER DISCIPLINES

We have mentioned two such methods earlier, both in very early stages of development. Both should be further examined and applied:

1. Asymptotic matched conic expansion techniques.

2. Adaptation of quantum mechanical techniques. The study of Schroedinger perturbation theory is being pursued in-house. A wealth of other techniques and approaches are awaiting study.

It should be clear from this discussion just what we have in mind on some of these items. Others may require further clarification. We are available for discussion of these and other pertinent areas for research at any time. As mentioned earlier, we expect our research program to be a flexible and growing effort. We feel that the outline presented here is a reasonable first iteration in the development of a comprehensive program. It is our hope that we shall have an extensive and continuing interchange of ideas with the scientific community, resulting not only in the elaboration of the ideas suggested by these problems but also in the formulation and incorporation of additional problems into a coherent research program designed to meet the needs of the future and finally in the implementation of the program on as broad a front as possible.

In closing, I should like to say that while I have necessarily dwelt on the deficiencies of existing theory, since these necessarily constitute the basis of a research program, it is far from my intent to, in any way, belittle the structure as it has grown from the past to the present. Celestial mechanics has, after all, commanded the best talents of the times from the earliest workers in the field right down to the present day and provides a challenge of the highest order for the future.

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GUIDANCE THEORY AND TRAJECTORY ANALYSIS CONFERENCE

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RESEARCH IN ASTRODYNAMICS AND COMPUTER EXPLOITATION

by

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INTRODUCTION

My purpose is to discuss the scope of research falling essentially within the area of numerical analysis as it relates to the Guidance Laboratory studies in Guidance Theory & Trajectory Analysis. Our objective is to conduct analytical research in both mathematical and computational methods for studying guidance theory and procedures for mechanization. To accomplish this, we have formulated a research program which includes developing new computational tools and procedures for identifying guidance system performance requirements, conducting error analyses and generating reference trajectory information.

The primary concern of this discussion is with the applications of the research effort. One of the applications of our research is supporting the NASA Headquarters Office of Space Sciences & Applications, and the Office of Manned Space Flight in their planning for future missions. In my presentation, I will pursue the thesis that the reliability of future missions may be increased by a comprehensive research program which in this case is in the numerical analysis aspects of Guidance Theory & Trajectory Analysis. Notwithstanding the customary reliability considerations associated with hardware, research of this nature is important since it often has a direct effect on the design and development of hardware and its performance provided this research sufficiently precedes the demand for application.

Here I would emphasize that the nature of our basic research is envisioned as long term. However, utilization of the continuing development of this research constitutes a different problem. In this regard, tapping the research at any time and using any developments applicable to the problem or mission at hand will be our goal. The topics I plan to cover in developing research needs are: Astrodynamics, Computer Exploitation, Orbit Computation, Computer Programs, Approximation of Functional Models, and Automatic Symbol Processing.

ASTRODYNAMICS

We understand astrodynamics to be that branch of astronautics whereby an engineering or numerical approach is taken to gain information on problems of a celestial mechanics nature. Generally, this is achieved by generating numerical solutions to differential equations and building from single case solutions an overall view of the given problem.

Astrodynamics and computer exploitation as disciplines are in some ways inseparable. Due to the vast amount of numerical data encountered in a characteristic astrodynamics problem of a celestial transit, the current state of the art techniques in computer exploitation are inadequate for sophisticated analysis imposed by future demands. However, computer exploitation is necessary for economic development and implementation of all research. In varying degrees, this relationship and interdependence manifests itself in all the mathematical disciplines that are being discussed here today.

In this section, I will discuss our viewpoints on astrodynamics in particular and then in a later section concentrate on computer exploitation in general. As you may have gathered, astrodynamics is a many splintered field encompassing studies of reference trajectory determination in the n-body problem on the one hand and on the other, extending to rather simplified problems in the two-body sense. Our interest in astrodynamics lies primarily in the initial value, boundary value and stability problems of celestial transits and orbit transfer which arise in identifying guidance system performance requirements, conducting error analyses and generating reference trajectory information. As indicated, in astrodynamics, a given problem is attacked by generating a number of single case solutions. After having generated a coherent set of solutions within the mathematical continuum of solutions, certain parameters are normally gathered and displayed in the form of tables or graphs. This treatment of the material although complete for the purposes inteded is of such a form that none but the most intimately involved completely understands or utilizes the information. This situation, we feel, exists not only due to the format of presentation, but also because the scientific community making occasional use of the material does not possess a basic concept of the flight mechanical features which would facilitate use and understanding.

Our proposal involves an approach to astrodynamics problems created by this lack of basic understanding. This proposal is not intended to supplant but rather supplement the tabular and graphical treatment of the astrodynamics problems. The concept we would employ is a geometrical or topological treatment of certain trajectory features. This results in pictorial or mapping illustrations which, when properly presented, enhance the understanding of the subject matter.

A reference for the technique of which I speak is the article presented in AIAA, Astronautics & Aerospace Engineering, February, 1964, entitled "Mapping The Course For The Moon Trip." An additional contribution of this study effort is that it has indicated possible functional relationships between certain initial and terminal parameters. Likewise similar topological mappings exist for intermediate points on these transits. One of the first research tasks that lies ahead for us is the determination and empirical fitting of the functional relationships which have been found. Material of this nature could prove invaluable for providing cutoff surfaces to powered phases which are required to meet the two-point boundary value solution. In like manner, a second important research need attempts to survey and classify interplanetary transits. Such an effort would aid tremendously in the development of guidance requirements.

The flight mechanics of orbit transfer, both impulsive and continuous thrust, constitute a third research need. In this problem area, a great deal has been accomplished in planar analysis and many other studies are currently in progress, but the greatest need seems to lie in the area of three-dimensionality. A recent accomplishment of note is that of Breakwell and Rauch in formulating an approximate analytic solution to the planetary spiral portion of continuous or low thrust trajectories; however, precision numerical means of orbit computation need more work. Our appeal is intended to stimulate interest in three dimensional orbit transfer analysis and encourage speculation on new methods and techniques which would appreciably elevate this area of research.

COMPUTER EXPLOITATION

Computer exploitation (or utilization) as used and understood here is the judicious use of large scale modern electronic computers and the current computer technology for the solution of problems in the fields of trajectory analysis and guidance theory development. The importance of this exploitation in efficiently implementing research findings has already been indicated, so I will now endeavor to indicate some areas where the extension of numerical analysis techniques would be desirable. The process of generating reference trajectories and some guidance functions places heavy demands on accuracy and on computer time. This is a result of the complexity of some perturbation equations and the extended transit times required for some orbit transfers and celestial missions. On the one hand, the minimization of computing time is important because it allows the researcher to accomplish more with his available time, and on the other hand accuracy is obviously necessary for installing confidence in results. It is readily admitted that other factors enter into accuracy determination, such as the knowledge of certain constants. Even with this consideration, it is not believed that current day numerical integration techniques will be sufficient for application to future orbit computation problems. Significant extensions of numerical integration techniques would be a fourth important research need.

One suggestion would concern the use of an adaptive integrating technique. By this I mean a system of computer logic whereby the particular numerical integration method which is best suited, in some sense, to a given segment of a compounded celestial trajectory should be used.

Automatic Step Size Adjusting may assist in the control of accuracy. Such systems normally require tradeoff between speed of computation and error control. Here again some research is needed to extend and develop these techniques. Other possibilities in the same general area are the development of efficient higher order integration procedures and certain series expansion methods.

ORBIT COMPUTATION

The inability to find analytic solutions in the general n-body model has made it necessary to develop many numerical methods for integration and handling of equations of motion for orbit computation. The methods of numerical integration are many and varied and therefore no attempt will be made to review them here. Even though the state of development in numerical integration techniques is well advanced, research in this area is of major concern to us in view of the anticipated effort in interplanetary trajectories, orbit transfer, and guidance equation development.

In orbit computation, the manner of treating the differential equations of motion is also pertinent to our research effort. One of the more classic and straight forward methods is Cowell's. This method of calculating orbits involves the direct integration of the total acceleration, central as well as perturbative, acting on a space vehicle. It was first applied to the determination of the orbits of the eight satellites of Jupiter. Then, of course, we have the Encke's method of orbit calculation. This method of calculating orbits differs from Cowell's in that differential accelerations due to perturbations are numerically integrated and not the total accelerations. In essence, the deviations from a two-body reference orbit are calculated.

These as well as many other prominent methods have been sharing the burden of responsibility in orbit calculations. They have no doubt performed satisfactorily for their purposes, but it is felt that more is needed for future research. The anticipated applications involve flights of long duration where current methods do not provide sufficient accuracy nor desired speed. Our hope would be that efforts in this fifth area of research might lead to an improved method of orbit computation which satisfies both increased accuracy and speed of computation for long term n-body trajectories.

COMPUTER PROGRAMS

In the area of computer programs, we plan a significant amount of research and development.

As a young organization, our library of computer programs is still small; however, we fully intend making use of the many and varied programs available from NASA centers and other sources throughout the country.

With respect to computer programs, our philosophy includes a preference for utilizing a family of computer programs which stem from a unified heritage. Thus, we should be able to obtain more knowledge and control of the quality and efficiency of output. This is especially desirable in situations where more than one computer program is needed on a problem. The transferral of data is facilitated when there is some uniformity between various systems and parameters.

In our efforts to develop, or have developed, computer programs to conduct and implement research, we would pursue the standardization of computer programs, the use of a standardized computer programming language and modularization of component systems whenever possible. The standardization of computer programs is an area in which we intend taking an aggressive lead. These efforts would place economy at a premium, require the use of the International System of Units, and require the use of the physical constants being used in the Apollo Program until they are replaced by a more accurate and consistant set. The programming language we favor for our research and operational decks is FORTRAN - IV. It is most compatible with our systems and seems to offer sufficient flexibility for incorporation of future modifications. The concept of modularization is a carry over from the subroutine concept. Subroutines are generally segments of computer programs that perform a single or limited number of mathematical operations when called upon to do so. Modularization would carry this one step further, where whole computer programs, simple as well as complex, would be made interdependent and in a like manner be called upon when their particular function was needed. The desire for modularization stems from the increasing complexity of compounded space missions.

Since this concept appears to be relatively new, the opportunity for research in this sixth field is practically unbounded.

At this time, we find ourselves particularly in need of performance and simulation decks. In the design of these computer programs, we would plan to uniformly bridge the gap between large effect models for rough approximations and models where maximum achievable precision is assured. Care should also be exercised to assure that reasonable bounds of applicability are built into most of these computer programs.

Within our research area, we plan to develop a set of computer programs that will possess varying degrees of economy (machine time), accuracy and complexity, such that an appropriate selection may be made for any application. Of course, such a goal is possible only with proper documentation accompanying the development.

APPROXIMATION OF FUNCTIONAL MODELS

It should be evident that a need exists for research in approximation theory of multivariate functional models. This seventh research need stems from the fact that analytical solutions are not available for many of the problems encountered in the development and implementation of guidance equations.

Guidance considerations of optimal flight imposed on the analysis of celestial transits results in a general two-point boundary value treatment of most problems. In this connection, there is only one optimum transit for a given mission, if indeed a solution does exist. In order to closely adhere to the optimum, engineering consideration requires the cognizance of a coherent class within the area of optimum flight as the ensemble from which a selection is made. Of course, this is changed in breadth with widening launch windows and consequential opening up of initial conditions. If one desired a functional representation of a particular surface within this manifold of trajectories, he would have no recourse other than to use multivariate approximation theory. Applications to space flight would require the "best" possible approximation. By the "best" approximation is meant the achieving of a maximum accuracy in some qualified sense. We are of the opinion that many of the qualifications may be reduced and the degree of accuracy improved as a result of research in this area of statistics.

Additional problems in this field that merit particular attention are determining optimal point or data selection from a given sampling, selection of the best functional form for a given application and exploiting the full potential of Chebychev and Least Squares approximations.

AUTOMATIC SYMBOL PROCESSING

The eight area where research is required is Automatic Symbol Processing. This is defined as the field of techniques that utilize large digital computers to perform general algebraic and differential operations. Several techniques have been developed during recent years, and have reached the stage of useful application. Some of the more prominent of these programming systems go by such names as ALGOL, FORMAC, and ALPAK. They have the characteristic of performing symbolic mathematical operations, making it possible for them to be used as tools in assisting the development of guidance equations, perturbation theory and in many problems where the speed and capacity of large scale computers allow research that was heretofore considered impractical. From the applications viewpoint, it should be noted that these systems are not restricted to symbolic manipulations. Numerical inputs can be accepted and coefficients or even a single numerical value can be generated.

Even though Automatic Symbol Processing potentially has a bright future, there are problems within the systems. It would seem that such problems might best be overcome as a result of a combined effort between the researcher or user and the developers of these systems. The difficulties we have encountered in the use of some of the ASP systems have been: first, the logic with such systems is not of sufficient status that they are able to perform all operations called for; second, for problems of notable size (perturbation series expansions) computer storage is taxed; third, the collection or recombination of like terms upon completion of the symbolic operation is not fully satisfactory.

However, we are very optimistic about the potential of ASP and feel certain that its effect on basic research and its application will be almost unlimited.

In conclusion, I might summarize the research tasks we plan to pursue in the near future.

1. General functional representations of the characteristics of celestial transits taken in the large.

2. Geometrical or topological survey and classification of interplanetary transits.

3. Three dimensional orbit transfer studies.

4. Improvements in numerical integration techniques--adaptive integration techniques, automatic step size adjustments, higher order integration schemes.

5. Improvements in orbit computation methods.

6. Research and development of modularization of computer programs.

7. Research to advance multivariate functional model approximation theory; optimal point selection; best functional form and exploitation of Chebychev and Least Squares techniques.

8. Research to advance the techniques of automatic symbol processing.

These tasks are by no means all inclusive and are only viewed as our initial effort. We would recommend and solicit ideas from you for tasks that, in your opinion, would materially assist our research in Guidance Theory and Trajectory Analysis.

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GUIDANCE THEORY AND TRAJECTORY ANALYSIS CONFERENCE

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RESEARCH IN OPTIMIZATION AND GUIDANCE THEORY

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The problems that I will be discussing under the heading of "Optimization and Guidance Theory" touch those that have been discussed by the preceding speakers at many points. However, there is one feature that is focused upon by the problems of the present category, and that is the presence of a force vector that can be varied in magnitude or direction. The optimization problem asks the question of the best possible way of varying this force vector under given circumstances so as to result in a motion of the flight vehicle that meets certain objectives. The guidance theory problem is to find ways of making that information available in a form that enables such optimal paths to be actually followed during flight, at least to a degree of approximation that is well controlled.

It is our intention to maintain a capability to derive and compute answers to specific problems in these areas as they are needed to support other programs of ERC and NASA. Thus, we will need to be able to compute optimal trajectories for most of the choices of optimality criteria, constraints, vehicle design parameters and mission requirements that are likely to be of interest, and also to derive and evaluate mathematical procedures for guiding the vehicle throughout its flight. For some classes of choices for these specifics, methods for obtaining the desired information is presently available, at least to some degree of approximation, and _if_ we assume there is always ample time and computer capacity available.

However, for most of the more complex missions of the present and the future, these present methods involve various arbitrary assumptions for which the performance penalty is not controlled or known. The methods usually have also been refined for one very restricted application and must be seriously modified to yield information for a slightly different problem. Our research program will then have as its goal the development of theory and techniques that can provide this type of information more efficiently, or that can provide the information for problems of interest that can not presently be handled. By more efficient methods, we mean methods that are faster for a given accuracy, require less computer capacity, and are applicable to larger classes of problems. The extent to which this improvement can be made then depends on the mathematical nature of the physical problems, and man's ingenuity in solving them.

We feel that the greater advancement is likely to come from covering all reasonable approaches that are intended to solve relatively broad classes of relevant problems, rather than concentrating upon very particular problems. Thus we will list some of the problem areas that need to be studied, according to the approaches that seem most likely to give results.

The optimization problem is being attacked by three major approaches. These are the classical calculus of variations, the modern calculus of variations, and numerical methods.

Most of the work done from the classical approach was done before there was any concern for optimum rocket flight, and is fairly well summed up by the work of Bliss and his students at the University of Chicago. A good example of how this work may be applied to such problems is contained in recent work by Boyce and Linnstaedter. They have extended the work of Hestenes, Denbow, and Valentine to obtain the first three necessary conditions for a multistage Bolza-Mayer Problem involving control variables and having inequality and finite equation constraints. This theory can usefully be applied, for example, to the problem of optimizing the entire trajectory of a multistage vehicle from lift-off on earth to injection into a lunar satellite orbit. The individual pieces of this problem have been treated fairly well, but a method for computing a truly overall optimum trajectory has not yet been perfected. Another problem to which the wealth of classical literature might be applied is that of mapping regions of phase space reachable by an optimally thrusting vehicle, by evaluation of sufficiency conditions. Thus our first problem area might be referred to as exploitation of the classical literature.

Some problems, however, just don't seem to fit well in the classical formulation. Since the variables are never precisely known during flight, one may ask what effect the stochastic nature of the variables has on the optimality of the trajectory. Also, since optimal trajectories cannot be followed exactly in practice, there may be some profit derived from the study of approximately optimal trajectories, where the degree of approximation is to be controlled or at least known. These problems seem to fit better into the language of functional analysis and the modern calculus of variations, which makes up the second approach to optimization problems, and defines a second problem area.

The third approach to optimization problems is the numerical approach. The gradient approach falls in this category, in which the function to be optimized is worked with in numerical form and the solution is approached stepwise.

This brings up the two aspects of optimization problems that our research must attack. One is the theoretical determination of conditions that are necessary, or sufficient, or most ideally necessary and sufficient, for a trajectory to be optimal, or optimal within some known degree of approximation. The other is the conversion of these conditions into a useful form to provide solution trajectory information.

One approach to making this conversion is to solve the two-point boundary value problem produced by either classical or modern variational theory. One promising current method for doing this is the Newton-Raphson procedure generalized through functional analysis viewpoints. We might classify the area of numerically computing solution trajectories as our third problem area, with emphasis on multiple phase trajectories, and keeping in mind the desirability to be able to perform such computations on board.

The problems in guidance theory are necessarily related to trajectory theory in that any given guidance policy must at least be able to result in a trajectory satisfying mission conditions. How close to optimal the resulting trajectories are may be taken as one measure of goodness for that particular guidance policy.

The approaches to obtaining guidance solutions that we will be studying fall into two main categories. The first takes the point of view of simplifying the physical problem so that it may be solved in closed form, and in such a way that the simplified problem approaches the physically real problem as the point of mission satisfaction is approached. The so-called explicit guidance modes fall in this category. The second category contains approaches that leave the problem in a relatively complete form, and then approximate the guidance form solution to the two-point boundary value problem that results from application of variational theory.

The second category can be further broken down into numerical and analytical approaches. By the numerical approach, one computes representative values of the function over the appropriate range of phase space, and then approximates that numerical sample with a multivariant function.

The problems of representing and fitting such functions were covered in the problem areas of computer exploitation.

One analytical method of the second category is to formally solve the differential equations of motion and variation in series form. By reversion of these series together with the mission equations, series form solutions for the guidance variables may be obtained and evaluated for unimportant terms that may be neglected. A second method that will be continued expands the two-point boundary value problem in a Taylor's series about a known solution point. All the coefficients can be determined exactly, by solution of linear systems of equations, and numerical integration of the proper quantities. Very similar to these approaches are the so-called second variation methods and their modifications, where higher order terms are brought in over the framework of optimal linear feedback theory.

These three approaches serve to illustrate a fourth problem area, the development of methods for obtaining analytical guidance form solutions to the two-point boundary value problems associated with optimum propelled flight. We feel that effort in following such approaches as these is well expended, since analytic solutions to more realistic optimization problems than are now available could be profitably fed into all the other approaches. They can also provide better functional models with which to fit numerical guidance solution points that have been generated from accurate physical models.

Another approach to analytical solutions that has only recently been attempted is the general perturbation methods of celestial mechanics. There is much investigating to be done on the choice of steps to be taken in splitting the Hamiltonian, and in application of automatic symbolic processing to the large amount of algebra that is involved. The application of Hamiltonian theory to various phases of the calculus of variations makes up our fifth problem area.

A new perturbation theory has been used by Arenstorf to prove the existence of new periodic orbits. Its uses for deriving analytic or computational solutions to the variational equations has not yet been explored, and offers a sixth task area.

A considerable amount of routine algebra arises in many of these analytical approaches, which again touches the problem area of automatic symbolic processing. We would be interested in its applications to the large amounts of algebra and possibly broader categories of reasoning that are involved in some of these approaches to optimization and guidance theory.

To further give you examples of how these problems may be approached with specific tasks, I will describe a few that we are currently interested in pursuing:

1. To develop new concepts and computer techniques for handling the large amounts of algebraic manipulation that occurs especially when dealing with series represeantations of functions.

2. To develop new procedures for solving problems related to constraints on trajectory shaping for complete missions and complete vehicle systems in an optimum manner.

3. To develop theory for minimizing the expected value of a functional defined by a system of differential equations whose parameters and initial conditions are stochastic variables.

4. To develop computer programs for solving special trajectory problems involving hardware constraints and proper coordinate selection.

5. To develop a Hamilton-Jacobi Procedure for deriving an analytical solution to the optimal trajectory equations.

The list of problem areas that has just been described is not intended to be definitive, but rather should serve as a general guide as to the type of work we are interested in doing and supporting. The statements have been made with a certain amount of freedom, so as to encourage you who may be interested in this field to suggest other approaches according to your ideas and interests.

In conclusion, let me offer a few guidelines to those of you who may at some time wish to formulate your ideas in any of the areas discussed today in the form of a proposal. First, the solution to a realistic problem of interest to us should be aimed at. If this is being attacked through some simplification, both the full problem and the simplified problem should be clearly defined. If only a portion of the problem is to be attacked, this should be noted. Or, if it is a new method of solution that is being investigated, this method and the assumptions that it involves should be defined along with the problem it is attacking and the problems upon which it will be tested. In any case, it should be indicated how the proposed problem or method is expected to eventually lead to a full solution of the first mentioned realistic problem. Such a problem definition should then remain invariant with respect to results obtained later in that the results should show to what degree the proposed problem was solved or approximately solved, or to what degree the method was fruitful or apparently not, rather than to represent the results as complete answers to modified problems. Let me add that I don't expect these statements to be completely applicable to all forms of research tasks that may be desirable, but simply represent some of the more common weaknesses that we have found from our past experience in the area. We hope that you will respond with many well thought out ideas that have the potential of adding significantly to the present state of knowledge in the fields that we have discussed today.

THANK YOU.